Dare one suggest that perhaps these "orchids" rather like going to shows?

It is, of course, very easy to criticise and very difficult to construct. It would be presumption on my part to suggest how the Association should proceed to preach the good tidings to all the world, but no one can doubt that the thousand or so of scientific men who attend the meetings year by year could do a great thing for humanity if they chose, and could very soon discover the best means of doing it also. But the methods of the days when, as one of your correspondents recalls, crowds used to appear wherever Huxley was expected to speak are certainly ineffective to-day. To begin with, I am not aware that we have any Huxleys nowadays, and if we had I doubt if they would have that sort of hold on the common or British Association public of to-day. Scientific men should be the last to try to put new wine into the old bottles; it should be an easy matter for them to devise newer and better bottles.

An old member of the Association said to me during the Cardiff meeting: "Well, you may say what you like about it now, but it's a kinema compared with what it was when I first knew it!" I was reminded of this by a chance paragraph in the newspaper the other day describing "the most remarkable film which has ever been made in France," called "Les Mystères du Ciel," and there is no doubt that it made an impression on the Parisian correspondent of the *Observer*, who belongs to a profession which takes a lot of impressing. He lays stress on the fact that some of "the best-known experts have been glad to assist in the making of this remarkable and ingenious film, which has a real educative value." I suppose this will horrify some of our revered seniors, but really the British Association might do worse than kinematise itself a bit further. After all, rightly or wrongly, the kinema does attract and instruct the people more than the Association does, and it certainly collects their money.

R. V. STANFORD.

Radyr, October 23.

Testing Einstein's Shift of Spectral Lines.

I AM not aware that anyone has applied centripetal acceleration to the outstanding Einstein prediction, instead of depending on solar gravitation. It is feasible to whirl a steel disc, 1 metre in diameter, at 3000 revolutions a minute; and this gives a peripheral acceleration five thousand times earth-gravity, whereas solar-gravity is only 25*g*.

A few vacuum tubes braced to such a disc would give the effect of continuous illumination; and someone with refined spectroscopic appliances may be willing to try the experiment—unless there is a fallacy in the suggestion.

OLIVER LODGE.

Gullane, October 20.

Recapitulation and Descent.

IN NATURE of October 14 my friend and colleague, Mr. L. T. Hogben, contributes a thoughtful letter on "Recapitulation and Descent," on which you will, perhaps, allow me to make one or two comments. Mr. Hogben traverses the position taken up by Dr. Bather in his address to the Geological Section of the British Association that "recapitulation" in the development of animals is a proof of evolution. His objection is that "experimental breeding" does not justify the inference that a mutant recapitulates the characters of its ancestral type, and that "factorial omission" rather than "the perennial desire of youth to attain a semblance of maturity" is the key to "the omission of some steps in the orderly process."

Now I fully agree with Mr. Hogben that if by "experimental breeding" and "genetic investigation" we mean the endless and wearisome repetition of the crossing of "Mendelian mutants" with one another and with the parent species, we shall wait until doomsday before we obtain any light on recapitulation, or, indeed, on any of the other broader aspects of the evolution theory.

When the upholders of a theory confess, as does the leading British Mendelian, that it is totally unable to throw any light on the origin of adaptation—which is, after all, the very heart of evolution—the biologist must indeed regard it as bankrupt, at any rate if it claims to be a full exposition of heredity. Not all "genetic investigation," however, is of the Mendelian type, and quite recently some patient researchers claim to have accomplished something like evolution on a small scale experimentally, and to have found traces of recapitulation. I am aware that these results have been regarded by Mendelians with scepticism, as I think quite unfairly; but until the Mendelians have repeated the experiments and disproved the results, these results must stand as the relevant facts. They are beginning to come in from widely divergent sources, and the easy method of getting rid of them by doubting the *bona fides* of the researcher is no longer available.

The great evidence in favour of the reality of recapitulation is that in our survey of the animal kingdom we encounter facts which literally compel every naturalist who encounters them to interpret them in this way and no other. When, for instance, we find a tortoise with a soft, flexible skin devoid of the bony plates which support the carapace of all his brethren, and it transpires that this tortoise enters on his career as an ordinary tortoise with a regulation carapace, what other explanation than recapitulation can be possibly entertained? When, further, we find that Cœloplana, which looks like a flat-worm, and Tjalfjella, which resembles a Sponge or an Ascidian, both begin their free life as exquisite little Cydippid Ctenophores, does anyone consider it possible to doubt recapitulation, and therefore evolution? This may not be logical, but it is convincing, and as Huxley long ago said: "If a man chooses to maintain that a fossil oyster-shell is a concretion, and not the remains of an organism, it is impossible to drive him from his position by logic."

I differ totally from Mr. Hogben in believing that the "omission of factors" has anything to do with the shortening of the developmental process. Rather I am convinced that this shortening is akin to the greater quickness with which an habitual act is performed after countless repetitions. If, for instance, we compare the degenerate eyes met with in the pathological cripples known as Mendelian recessives with the degeneracy due to loss of function owing to changed habits, we meet with a totally different picture in the two cases, as anyone consulting the literature can see.

Nor can I agree with Mr. Hogben that much of the reasoning of the past originated in an emotional recoil excited by popular prejudice. The reasoning of the past reflected the burning impression created by the impact of myriads of new and unsuspected facts, and we owe Dr. Bather a debt for pointing out in his brilliant address that the old methods are perfectly sound *if properly applied*. At first they were applied in a wild and careless manner, and hence the reaction against the doctrine of recapitulation which set in, and of which Adam Sedgwick in his later years was a victim. But this reaction was no more justified than would be a reaction against Egyptology because some of the earlier Egyptologists drew rash conclusions from insufficient facts and sketched out

© 1920 Nature Publishing Group

fascinating hypotheses which later proved to be baseless.
E. W. MacBride.

Imperial College of Science and Technology,
South Kensington, London, S.W.7, October 22.

British Laboratory and Scientific Glassware.

The question of the manufacture of laboratory and scientific glassware in this country is now receiving the attention of the House of Commons, together with other key industries. This matter has been the subject of considerable correspondence in Nature, and I feel that readers of this journal will be interested to know what progress has been made in this work.

The advance made in the manufacture of laboratory and scientific glassware in Great Britain during the last five years is not only remarkable, but is also a monument to the capacity and ability of the British scientific worker. It is unnecessary to point out here the importance and necessity of scientific work or of the vessels and instruments that men of science use. That the latter must be of the finest quality and manufacture is indisputable.

At the outbreak of war men of science were absolutely dependent on supplies of German glass. They realised the danger, and at once stepped into the breach that the British manufacturer, unaided, was unable to fill. As a result of their efforts we now have the nucleus of a considerable industry in this country. While the supply is not yet equal to the demand, rapid progress has been, and is being, made.

Men of science are always critical of one another's work, and the production of these laboratory glasses has naturally led other workers to test and compare their properties with those of German and other makes. These results have been published in purely technical journals, but I feel that they have not yet had the publicity given to them that they deserve. The results of these tests are really remarkable, and prove quite conclusively that, in spite of the short time that the industry has been established, British laboratory glassware is the finest in the world. The results have never been challenged in any way, and go to show that the widely and cleverly advertised properties of German glassware are not quite so good as they have been made to appear, and that the British manufacturer has not merely equalled their best, but surpassed it.

The reports of the series of tests referred to are contained in the Journal of the Society of Glass Technology, the references being as follows:—Vol. i., p. 153: "The Attack of Chemical Reagents on Glass Surfaces, and a Comparison of Different Types of Chemical Glassware." Vol. ii., p. 219: "The Resistant Properties of some Types of Foreign Chemical Glassware." Vol. iii., p. 129: "Further Investigations of Chemical Glassware." These researches have been carried out by a department of Sheffield University.

The glasses tested have been classified (vol. ii., p. 230) under the headings "Good," "Moderate," and "Bad," and include Jena glass, Greiner and Friedrich's "R," Koln Ehrenfeld's, Swedish, Italian, French, American, and British. Of these only seven (two German, two American, and three British) remain in the "Good" class in every test. The tests include the action of (1) boiling water, (2) boiling water under pressure (autoclave), (3) $2N$-NaOH, (4) $N/10$-NaOH, (5) $2N$-Na_2CO_3, and (6) boiling HCl.

The action of boiling water at atmospheric pressure is so small on all seven glasses as to be, in the opinion of the authors of these researches, within experimental error, and is, therefore, negligible for comparative purposes.

An analysis of the table of results referred to above shows that for the remaining five tests the order of merit of the various glasses for general use is as follows: (1) A British glass. (2) A German glass. (3) A British and an American glass. (4) An American and a German glass. (5) A British glass.

This result speaks for itself, and it should be added that neither of the two German glasses in this list is Jena glass, which was found to occupy a comparatively low position in the table of results.

One of the great troubles which the manufacturer has had to face was that he was making this extraordinary attempt, not in normal, but in abnormal, times, when the supplies of raw materials and trained and unskilled labour were not available. Men skilled in the difficult art of "blowing" were almost unobtainable, yet these difficulties were gradually overcome and continued improvements made, until to-day the best British chemical glass bears comparison, from every point of view, with the products of the rest of the world.

In the early days many complaints were urged regarding the quality of the finish. This was only to be expected. It is obvious that skilled labour cannot be trained in a day, but I have no hesitation in saying that the finish of the majority of the best British makes of resistant glass is now as good as, if not better than, that of German glass of comparable composition.

One feels that the British manufacturer has at times been blamed for producing an inferior article. Unbranded glass has often been sent to chemists as British ware, whereas all the best makes of British glass are stamped with the name of the firm making it.

Unfortunately, those who were patriotic enough to manufacture this glass during the war are in danger of losing the result of their labours. Your readers know the heavy cost of experimental and research work, and will naturally realise that the British manufacturer is extremely anxious lest the results of this work should be permanently lost to the country. It is for this reason that the British manufacturer asks that the Key Industries Bill should be passed as soon as possible, to enable him to train more labour and to place this industry on a permanent and satisfactory footing.

S. N. Jenkinson,
President of the Society of Glass Technology.
"Rondels," Cookham Dean, Berkshire,
October 18.

The Behaviour of Time-Fuzes.

It might appear that in my article on "The Behaviour of Time-Fuzes" in Nature of October 14 I was describing my own researches. I wish to correct any impression of that kind. The original draft of my article was unsigned, but, unfortunately, I allowed the author's name (inserted by the Editor) to remain in the proof. The experiments described were made by a variety of people at Woolwich, at the National Physical Laboratory, at Cambridge, at University College, London, at Portsmouth, and elsewhere; and not least of the credit for the progress made in our knowledge of fuze-behaviour is due to certain officers of H.M. Army and Navy, on the Ordnance Committee, at the Ministry of Munitions, and at H.M.S. *Excellent*. My article, however, was intended not to apportion credit, certainly not to claim it, but to describe what I personally regard as the leading lines of development of a strange and interesting scientific by-product of the war.

A. V. Hill.

45 The Downs, Altrincham, Cheshire,
October 21.

© 1920 Nature Publishing Group